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THE HEAT CAPACITY OF ELECTRO-POSITIVE METALS AND THE THERMAL ENERGY OF FREE ELECTRONS

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According to the classical principle known as the equipartition of energy the kinetic energy of every kind of atom which is capable of motion in any direction is the same, and the change in this kinetic energy for a change in temperature of one degree is $3R/2$ calories per gram atom. If in a solid substance each atom instead of moving freely vibrates about a more or less fixed position according to the laws of a simple oscillator, it will possess on the average a potential energy equal to its kinetic energy, and, if we consider the simple case in which the volume is kept constant the heat capacity per gram atom should be equal to $3R$, or 5.97 calories per degree.

According to the empirical law of Dulong and Petit, the atomic heat capacity at constant pressure is the same for most solid elements and equal to a little over 6 calories per degree. Lewis[1] calculated by means of a thermodynamic equation the difference between C_p and C_v and found for a considerable number of elements that C_v is more nearly constant and that its average value at 20°C., when a few elements of low atomic weight are omitted, is 5.9, which is almost identical with the theoretical value.

Much recent work, both experimental and theoretical, has shown that the earlier formulation of the equipartition law can no longer be considered as universally valid but rather as the expression of a limiting law which approaches complete validity the higher the temperature, the heavier the atom and the weaker the constraints operating upon the atom.[2] Every factor which causes a deviation from the equipartition law leads to a smaller specific heat than the one calculated by that principle. If therefore we find for any substance a value of C_v greater than $3R$ per gram atom we are led immediately to consider the possibility that in addition to the atoms other particles, either inside or outside of the atoms, are acquiring thermal energy.[3]

In the calculations of Lewis to which we have referred the value of C_v was found to be 6.4 for sodium and 6.5 for potassium, but the difference between these values and the theoretical values might at that time have been ascribed to experimental error. More recently Dewar[4] has determined the mean atomic heats of a large number of elements between the boiling points of hydrogen and nitrogen. He found that at these low temperatures, where for most elements the atomic heat is only a small fraction of $3R$, that of cesium is 6.3. This is the value of C_p, but it seems likely that at such a low temperature the difference between C_p and C_v is very small even for an element like cesium. These facts led us to suspect that an abnormally high value of the atomic heat capacity might be a characteristic property of highly electro-positive metals. We were therefore interested to find that the experiments of Nernst and Schwers[5] on the specific heat of magnesium showed for this metal a change of heat capacity with the temperature which at low temperatures corresponds with that obtained for other monatomic elements,[6] but which at higher temperatures appears abnormally large.

In order to test the supposition that all electro-positive metals would exhibit this kind of abnormality we have determined over a wide range of temperature the specific heats of two metals of the first group, sodium and potassium, and of two metals of the second group, magnesium and calcium. The detailed measurements and the experimental method will be described shortly in the *Journal of the American Chemical Society*. Here we shall give only the general results of the investigation.

Before discussing the heat capacities found for the four metals it will be well to review briefly previous work on the heat capacities of other typical metals. Of these aluminum, copper, zinc, silver, mercury (solid), thallium and lead have been fully investigated over a wide range of temperature. For all of these metals C_v has been found to be the same function of T/θ, where T is the absolute temperature and θ is the temperature at which C_v is equal to $3R/2$. In other words, if we follow the procedure of Lewis and Gibson and plot C_v against log (T/θ) the individual values of C_v for all these metals fall upon the same curve, which they speak of as the regular curve, or the curve of Class I. This may be expressed in still another way by the statement that if the values of C_v as ordinates are plotted against the values of log T as abscissae the curves for the several elements may be made to coincide by horizontal displacement.

The form of the regular curve is shown by the continuous curve of figure 1, where C_v is plotted against the common logarithm of the temperature, and the individual points are those found for magnesiun. The five lowest points are taken from the measurements of Nernst and Schwers[7] and the remainder represent our own measurements. It is evident that at low temperatures the specific heat curve for magnesium coincides with the regular curve, but that at higher temperatures the specific heat rises more rapidly than in the case of any of the other metals to which we have referred.

That this same phenomenon is exhibited by potassium, sodium and calcium is shown in figure 2, where the regular curve which would fit the experimental points at lower temperatures is drawn as a continuous curve and the individual points represent our experimental determinations of the heat capacity. In each of these cases the heat capacity rises well above the theoretical value $3R$.

The deviation from the regular curve at any one temperature is greater for calcium than for magnesium and greater for potassium than for sodium. We may therefore predict that this phenomenon is more marked the more electropositive the metal, but that it will be found for every substance at high temperatures. According to modern theories of atomic structure the atom can

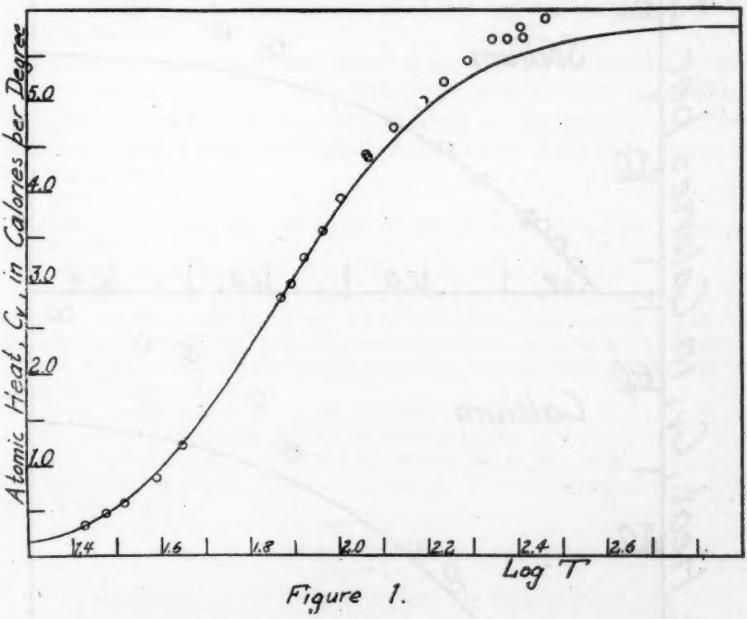

Figure 1.

no longer be regarded as the ultimate unit of matter. Besides the atomic nucleus there are electrons, and all of these particles, if capable in any degree of independent motion, should according to the earlier theory of the equipartition of energy, acquire their full share of thermal energy. If, however, we admit that at a given temperature deviations from the equipartition principle occur to an extent which is greater the lighter the particle and the more rigid the constraint which holds it to a fixed position, then it follows that at a temperature at which other particles are gaining thermal energy approximately according to the equipartition law, the electron which is by far the lightest of all particles, will acquire very little energy unless bound by very weak con-

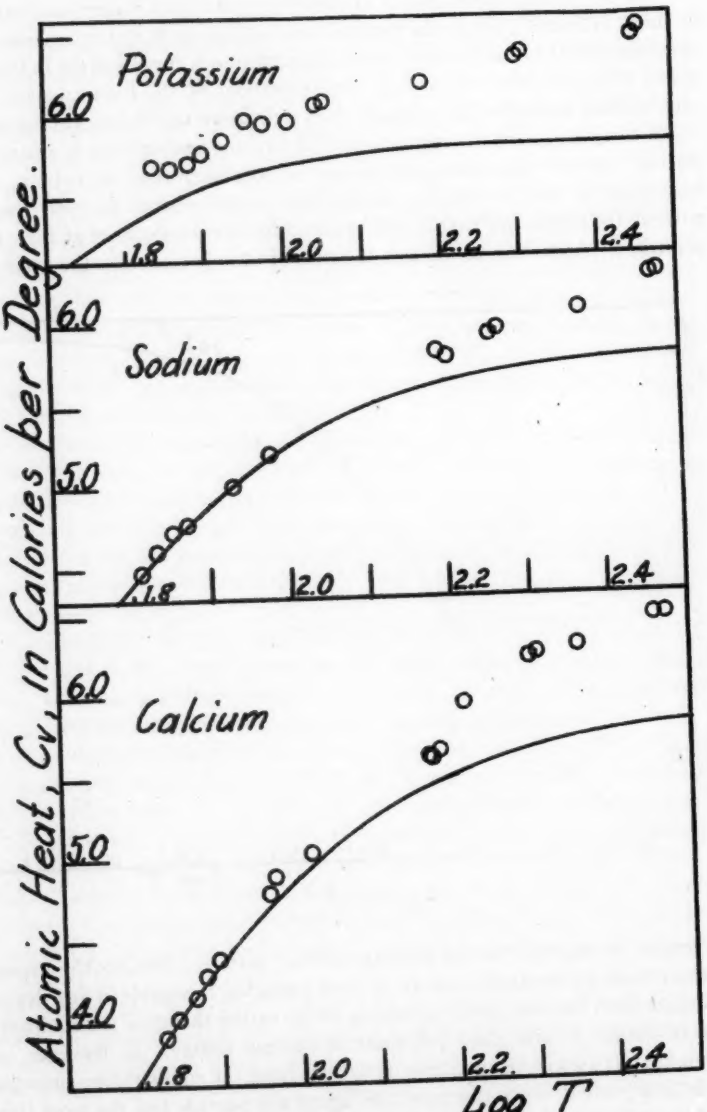
Potassium
Sodium
Calcium
6.0
5.0
4.0
1.8
2.0
2.2
2.4
Atomic Heat, C_v, in Calories per Degree.
Log T

Figure 2.

straints. The optical properties of most substances show their electrons to be held rather rigidly, but many of the properties of metals, and especially of metals pronouncedly electro-positive in character, indicate a high degree of electronic freedom.

It is therefore our belief that in the metals under consideration the difference between the heat capacity observed and that calculated from the regular curve, which fits the experimental curve at low temperatures, may be regarded as representing the actual heat capacity of their more loosely bound electrons. Whether these electrons are 'free' in the sense that each electron occupies a position symmetrical with respect to two or more atoms, or whether they remain attached to individual atoms, we should expect them to add to the heat capacity of the substance, provided that they are held by sufficiently weak constraints.

We have thus an entirely new method of investigating the freedom of electrons in a metal, and it is to be hoped that when further quantitative data are available a comparison of the results obtained by this method with those obtained through a study of the photo-electric effect, or the Volta effect, will prove of interest.

[1] Lewis, *J. Amer. Chem. Soc., Easton, Pa.* **29**, 1907, (1165); *Zs. anorg. Chem., Hamburg*, **55**, 1907, (200).

[2] Lewis and Adams, *Physic. Rev., Ithaca, N. Y.*, (Ser. 2), **4**, 1914, (331).

[3] Langmuir, *J. Amer. Chem. Soc., Easton, Pa.*, **38**, 1916, (2236), calls attention to the possibility that the atom in a solid may not be vibrating in simple harmonic motion and hence that the potential energy may not be equal to the kinetic energy, as has been assumed. However, if this were the case the potential energy would in all probability be less than the kinetic energy and not greater.

[4] Dewar, *London, Proc. R. Soc., A*, **89**, 158, 1913, (158).

[5] Nernst and Schwers, *Berlin Sitz. Ber. Ak. Wiss.*, 1914, (1), (255).

[6] See Lewis and Gibson, *J. Amer. Chem. Soc., Easton, Pa.*, **39**, 1917, (2554).

[7] The four highest points of Nernst and Schwers have been omitted since they appear to us to be erroneous.

THERMO-ELECTRIC DIAGRAMS ON THE P-V-PLANE

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Read before the Academy, November 21, 1917

About thirteen years ago I made an ill-directed and unfortunate attempt to analyze the electromotive force of a thermoelectric circuit, that is, to identify and explain the local e.m.f's which, taken together, give the e.m.f. of the circuit as a whole. I am now making a new attempt. I know that my present effort is more intelligently directed than the former one, and hope that it will prove to be more fortunate; but I do not claim for it finality.

I shall in the present paper discuss the matter on the assumption that the 'free' electrons within the metals are the only ones moving progressively in the maintenance of a current and the only ones taking part in thermo-electric action. For the sake of generality I shall not assume that these electrons have the same kinetic energy as gas molecules or even have the same kinetic energy in different metals at the same temperature. In fact, I shall in using the equation

$$pv = GRT, \tag{1}$$

where v is the volume of a gram of free electrons within a metal and G is the number of electrons in this gram, assume that R may be a function of T and may vary from metal to metal. I shall follow common practice in supposing n, the number of free electrons per unit volume, to be greater in some metals than in others and to increase in all metals with rise of temperature. I shall omit, as being unnecessary for a general explanation of thermo-electric action, the assumption of a specific attraction of metals for electrons.

In a detached bar of metal α, kept hot at one end and cold at the other, there is a higher free-electron pressure at the hot end than at the cold end, partly because of the greater value of n, partly because of the higher average heat energy of the electrons, at the hot end. Hence there is a mechanical tendency of the free electrons to move toward the cold end of the bar. But it cannot be said with confidence that this tendency, if unrestricted, would result in a condition of equal pressure from end to end of the bar; for it is quite possible, as I have pointed out in a previous paper,[1] that it would produce the condition of equilibrium which holds in 'thermal effusion.'

I shall make use in turn of two alternative hypothesis;

(*A*) That the mechanical tendency is toward equality of pressure, represented by the equation

$$nRT = a\ constant,$$

from end to end of the bar.

(*B*) That the mechanical tendency is toward thermal effusion equilibrium, represented by the equation

$$n(RT)^{\frac{1}{2}} = a\ constant,$$

from end to end of the bar.

Under neither hypothesis will there be any sufficient movement of electrons to produce an appreciable alteration of n from its natural value at any point,—that is, the value it would have at this point if the whole bar were at the temperature of this point. For the free electrons are mingled, naturally, with an equal number of positive metal ions which are not free to move through the bar, and therefore these electrons are powerfully restrained from any large movement down the pressure gradient. The proper distribution of an

exceedingly small excess of them along the surface of the colder half of the bar, with a corresponding deficiency in the warmer part, is enough to balance the mechanical-pressure tendency and maintain equilibrium. Of course this equilibrium is a mobile one, individual electrons moving in either direction through any cross-section of the bar constantly, but with zero net result so long as the bar is isolated and of unchanging temperature-gradient.

The pressure gradient, and the balancing electric potential, will depend on the rate at which n increases with increase of T in this metal α. Let this rate be such that the changes of pressure and volume suffered by one gram of elec-

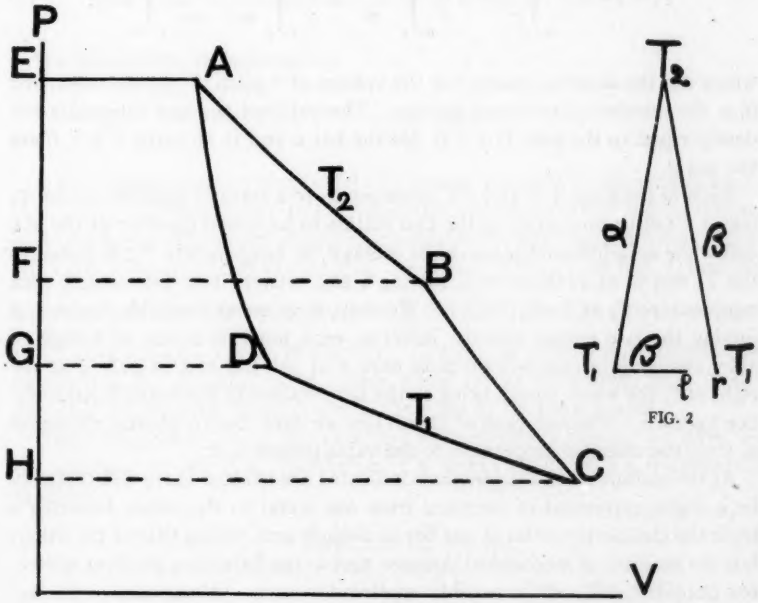

FIG. 1

FIG. 2

trons in passing, under conditions differing only slightly from those of equilibrium, from temperature T_1 to temperature T_2, or *vice versa*, through the metal α are represented by the curve[2] $A\ D$ in figure 1.

If this curve is steeper than the adiabatic curve of the moving body of electrons, this body will take up heat from the metal during its progress from T_1 to T_2; that is, the Thomson heat-effect in this metal will be of the same sign as that in iron. If the line is less steep than the adiabatic, the Thomson heat-effect will be of the same sign as that in copper. In lead the adiabatic line would be followed very nearly.[3]

Let the line $B\ C$ be for the metal β what the line $A\ D$ is for α.

Under hypothesis A.—In order to have electrical equilibrium within the bar, we must have

$$dp = f n\, dl \tag{2}$$

where f is the electrostatic force acting on a single electron in the direction of increasing T, and dl is the element of length of the bar corresponding to the change of pressure dp. Hence the decrease of negative electrostatic potential from the cold to the hot end of the bar is

$$P_1 - P_2 = \frac{1}{e}\int_0 f dl = \frac{1}{e}\int_0^l \frac{f n dl}{n} = \frac{1}{e}\int_0^l \frac{dp}{n} = \frac{1}{Ge}\int_{p_2}^{p_1} v\, dp, \tag{3}$$

where e is the electron charge, v is the volume of 1 gram of the electrons, and G is the number of electrons therein. The value of the last integral is evidently equal to the area $E\,A\,D\,G$ for the bar α and to the area $F\,B\,C\,H$ for the bar β.

Each of the lines $A\,D$ and $B\,C$ corresponds to a state of equilibrium for its metal. Let us now suppose the two metals to be joined together at the hot end while an additional piece of the metal β, at temperature T_1, is joined to the T_1 end of α, as in figure 2, leaving a gap between two pieces of β, with temperature T_1 at both l and r. We may, if it seems desirable, instead of joining the two metals directly, insert at each junction a bar, or bridge, of alloy changing in composition from pure α at the left end to pure β at the right end, the whole length being at the temperature of the α and β adjacent. See Figure 3. Through each of the bridges we shall have a gradual change of n, from the valuable proper to α to the value proper to β.

At the instant when the junctions indicated are effected there will probably be a slight movement of electrons from one metal to the other, lowering a little the electric potential of one bar as a whole and raising that of the other; but the gradient of mechanical pressure, and so the balancing gradient of electric potential, will remain sensibly unaltered in each. Along each of the isothermal bridges of alloy the general law of equilibrium will be the same as that which we have seen to hold in each of the homogeneous but now isothermal bars; that is, dp equals $fndl$. The volume-pressure changes through the bridge at T_1 will be those shown by the isothermal line $A\,B$ of figure 1; the corresponding changes through the bridge at T_1 will be those shown by the isothermal $D\,C$. The increase of negative electric potential through the bridge at T_2 from α to β will be, according to equation (3), represented by $\frac{1}{Ge} \times$ (*the area* $E\,A\,B\,F$). The corresponding increase at the T_1 junction will be $\frac{1}{Ge} \times$ (*the area* $G\,D\,C\,H$).

We still have equilibrium, the circuit being open at lr. But is the potential

of the β at l equal to that of the β at r? We can answer this question readily. Starting in α (fig. 3), close to the T_2 junction, going across the bridge and then down β to r, we have as the gain of negative potential

$$P_r - P_{\alpha_1} = \frac{1}{Ge}(EABF + FBCH) = \frac{1}{Ge} \times EABCH \qquad (4)$$

Starting again at the same point as before and going down α, then across the lower bridge to β, we get

$$P_l - P_{\alpha_1} = \frac{1}{Ge}(EADG + GDCH) = \frac{1}{Ge} \times EADCH. \qquad (5)$$

We have, then, by subtraction

$$P_r - P_l = \frac{1}{Ge} \times ABCD. \qquad (6)$$

If now we choose to connect l and r by means of some very large resistance, the current will flow from r to l through this resistance, while the conditions within α and β will remain almost unchanged. We shall have a thermo-electric current passing clockwise around the circuit of Fig. 2, or Fig. 3, and the e.m.f. maintaining this current will be $\frac{1}{Ge} \times ABCD$. The amount of electrical work done by a gram of electrons, Ge units, in passing once around this circuit is $ABCD$, which is, of course, equal to the mechanical work of the pressure-volume cycle through which the gram of electrons passes.

Under Hypothesis B.—In this case, as I have elsewhere shown,[1] we have, as the condition of electrical equilibrium in a detached bar with a temperature gradient,

$$dp = \tfrac{1}{2} p\left(\frac{dR}{R} + \frac{dT}{T}\right) + fndl. \qquad (7)$$

Hence

$$P_1 - P_2 = \frac{1}{e}\int_0^l fdl = \frac{1}{e}\int_0^l \frac{fndl}{n} = \frac{1}{e}\int_{p_1}^{p_2} \frac{dp}{n} - \frac{1}{2e}\int_1^2 \frac{p}{n}\left(\frac{dR}{R} + \frac{dT}{T}\right).$$

But

$$\frac{p}{n} = RT = \frac{pv}{G},$$

and so

$$P_1 - P_2 = \frac{1}{Ge}\int_{p_1}^{p_2} vdp + \frac{1}{2Ge}\left(p_1v_1 - p_2v_2\right). \qquad (8)$$

Graphically interpreted, equation (8) gives for the line AD, of figure 1 and figure 4, $P_D - P_A = \frac{1}{Ge} \times E'ADD'd'e'$, where E' is a point halfway between E and A, while D' is halfway between G and D.

For the potential difference of each of the lines $A\,B$, $B\,C$, and $D\,C$, of figure 1, we have a corresponding representation; but these areas all combined, the sum of the areas for $A\,D$ and $D\,C$ being subtracted from the sum of the areas for $A\,B$ and $B\,C$, will give, as before, $\frac{1}{Ge} \times A\,B\,C\,D$, as the net e.m.f. of the circuit.

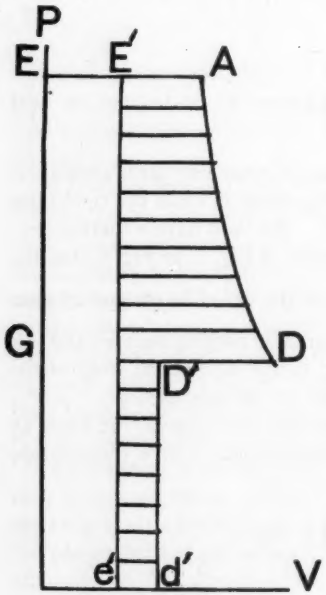

FIG. 4

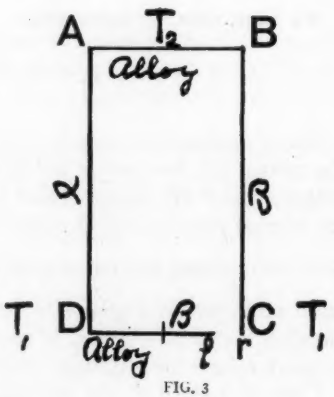

FIG. 3

If the diagram $A\,B\,C\,D$ represented the operation of an ordinary fluid, like air or steam, working in a cylinder under a piston, the path $A\,B\,C$ would represent that part of the cycle in which the fluid, expanding, does work on the piston at the expense of heat energy, while the path $C\,B\,A$ would represent that part of the cycle in which the returning piston does work in compressing the fluid. In the thermo-electric case, under the conditions which we have assumed, the path $A\,B\,C$ represents that part of the cycle in which the expanding electric fluid does work in storing up electric potential energy at the expense of heat energy, while the path $C\,D\,A$ represents that part of the cycle in which electric potential energy works to compress the electric fluid.

So far as mere expansions and contractions are concerned, we may ignore the electrical property of the circulating fluid; for the mutual electrical self-repulsion of its particles is balanced by the action of the equally numerous positive ions.

It is to be noted that no necessity has appeared in this discussion for any 'specific attraction' of either metal for electrons. Ordinary electro-static attraction and repulsion, together with the mechanical, expansive, heat-pressure of the electrons, is apparently enough to account for all that is known to occur in thermo-electric action confined to a metallic circuit. All the evidence we have to show the existence of the so-called 'specific,' or 'essential,' attraction comes from other regions of phenomena, especially from studies of thermionic emission and the Volta contact potential-difference. If such an attraction exists and is active within metals, we need not change the discussion above given except in the interpretation of P. This has here been taken as ordinary electrostatic potential. If there is an 'essential attraction' between electrons and matter having no electric charge, P will appear in the formulas just as it appears now, but it will be interpreted as what I have called in a previous paper[1] *virtual potential*, potential due to all attractions and repulsions acting on the progressive electrons.

It is of interest to observe that the e.m.f. along any part of the circuit does not, under either hypothesis (A) or hypothesis (B), necessarily correspond to the amount of heat absorbed in this part. For example, the Thomson heat absorbed along the line $A\ D$ may be positive, negative, or zero, according to the inclination of the line, while the e.m.f. of this part, represented by the area $E\ A\ D\ G$, remains always of the same sign.

The preceding discussion expressly assumes that the free electrons are the only ones which move through a metal. This is a provisional assumption only. If the associated electrons also move progressively, as I have in certain previous papers supposed them to do, the conditions of equilibrium in a detached metal bar, having a temperature-gradient, are different from those indicated in this paper. This matter I hope to discuss at another time.

[1] Boston, *Proc. Amer. Acad. Arts Sci.*, **50**, 4 July, 1914.

[2] Great steepness of this curve does not, as one might at first suppose, indicate a very rapid increase of n with increase of T, but the contrary.

[3] That is, if the moving body of electrons that constitute an electric current obeyed the laws of a perfect manotomic gas, we should have in lead $n \propto T^{1.5}$, very nearly.

A DETERMINATION OF THE SOLAR MOTION AND THE STREAM MOTION BASED ON RADIAL VELOCITIES AND ABSOLUTE MAGNITUDES

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In previous determinations of the motion of the sun in space and of the star streams the stars have been divided for discussion according to spectral type or apparent magnitude. The recent investigations of Adams and Strömberg[1] have shown that the intrinsically faint stars have a higher average radial velocity than those that are intrinsically brighter, or in other words, that radial velocity is a function of absolute magnitude. Accordingly an investigation of the solar motion and the stream motion based upon a division of stars into groups of nearly equal absolute magnitude is of exceptional interest, since the dispersion of the radial velocities within each group is considerably less than in the usual case. A brief account of such an investigation is given in this communication.

Of the 1300 stars of the spectral types F, G and K with measured radial velocities which have been used in the discussion, about 700 have absolute magnitudes determined spectroscopically by Adams. For the remainder, observed mainly at the Lick and Mills Observatories, mean parallaxes have been computed by the aid of the following formula connecting proper motion and apparent magnitude:

$$\text{Log } \pi = \log A + \log (\mu + c) + m \log \epsilon,$$

in which π is the mean parallax, μ the proper motion, m the apparent magnitude, and A, c and ϵ constants determined by means of the spectroscopic parallaxes. The formula differs from that of Kapteyn,[2] which for later type stars of very small proper motion gives parallaxes that are too small, in the addition of the constant c.

Solar Motion.—The constants of the solar motion have been determined by a least squares solution of equations of condition of the form

$$V = x_0 \cos\alpha \cos\delta + y_0 \sin\alpha \cos\delta + z_0 \sin\delta + K,$$

in which V is the radial velocity, $-x_0$, $-y_0$, and $-z_0$ the rectangular components of the sun's motion, and K Campbell's K-term, the quantity which must be subtracted from each of the radial velocities, corrected for the sun's motion, in order to make their sum equal to zero. The results indicate that the K-term has a small positive value in the case of the very luminous stars, and probably a negative value in the case of the fainter stars. For K equal to

zero the constants of the solar motion have the values given in table 1. M and m are the arithmetical means of the absolute and apparent magnitudes, A and D the right ascension and declination of the sun's apex, V_0 the sun's velocity in space, and θ the arithmetical mean of the radial velocities corrected for the sun's motion, taken regardless of sign. The results given by the groups of faintest magnitude are necessarily uncertain since the stars included within them are almost exclusively in the northern hemisphere and the distribution is relatively unfavorable.

TABLE 1

CONSTANTS OF SOLAR MOTION ($K = 0$)

NO.	$\overline{M}$	$\overline{m}$	A	D	V_0	ϑ
			F and G type stars			
					km.	km.
211	0.31	4.68	251°.4	+22°.5	19.4	11.3
177	1.44	5.42	267.5	+36.3	16.6	14.6
167	2.76	5.27	272.1	+36.4	22.8	16.3
170	5.29	6.41	(279.6)	(+10.9)	(27.1)	(23.9)
725	2.32	5.40	268.3	+26.1	20.1	17.2
			K type stars			
122	0.54	4.22	279°.6	+33°.8	24.0	13.7
245	1.41	4.86	268.1	+37.2	20.4	16.6
99	2.58	5.12	284.5	+20.1	26.0	18.6
79	7.07	7.41	(289.0)	(+26.5)	(22.1)	(26.2)
545	2.25	5.13	277.0	+32.5	22.2	18.5
			M type giants			
135	1.5	4.98	264°.2	+26°.1	26.8	16.9

All stars of late type

$A = 270°.9 \pm 3°.3$ $D = +29°.2 \pm 3°.4$ $V_0 = 21.48 \pm 1.02$ km
$\vartheta = 17.7$ km $K = +0.36 \pm 0.60$ km

There appears to be a tendency toward smaller values of the declination for the intrinsically faint stars. If real this effect may be explained on the assumption of a variation in the proportion of stars belonging to the two star drifts. A marked feature of the results is the increase of the average radial velocity with decreasing brightness.

Stream Motion.—For a study of stream motion the stars with measured radial velocities have been divided into three groups, as follows, according to absolute magnitude:

GROUP	NO.	$\overline{M}$	$\overline{m}$	π	ϑ
					km.
I	509	0.76	4.83	0".015	13.11
II	513	2.08	5.09	0.025	17.13
III	260	6.05	6.82	0.070	25.88

Each group contains stars of spectral types F, G and K, the third group, comprising the faintest stars, also including 11 stars of the dwarf M type. The symbol π denotes the geometrical mean of the parallaxes, which is readily derived by the formula

$$5 \log \pi = M - m - 5.$$

In accordance with the method of Charlier, the sky has been divided into 48 equal areas situated symmetrically with reference to the galactic equator. If star streaming is studied on the basis of the ellipsoidal or the two drift theory, opposite areas may be combined by assuming that the stream motion is the same in all parts of the space.

Since the sun, however, is not situated at the center of the stellar system, but at a distance from it probably comparable with the mean distance of the later type stars used in this investigation, we might, according to the theory of Turner[3] and Eddington,[4] conceive the stream-motion to be due to the general attraction of the stellar system. It would then differ from point to point and be related to the position of the center and the central plane of the stellar system. Such a theory is supported by Kapteyn's[5] suggestion that there is an acceleration of the first stream; i.e., that the velocity of the first stream is different in different parts of the space.

In order to test for the existence of such a varying stream-motion, I have tried to express the average radial velocity as a rational integral function of the direction-cosines of the line of sight such that the radius vector of the surface thus defined equals the average radial velocity. Using only terms of second order we can in this way determine the stream-motion, if the latter is constant (Eddington[6]). If the stream motion is variable this must be marked by the existence of asymmetrical (odd) terms in the analytical representation of the surface.

In this investigation terms to the third order inclusive have been determined.

If terms of the second order alone are included, opposite areas in the sky may be combined. The resulting directions of the axes of maximum radial velocity θ are thus:

GROUP I			GROUP II			GROUP III		
ϑ	α	δ	ϑ	α	δ	ϑ	α	δ
km.			*km.*			*km.*		
16.14	98°	+5°	20.98	86°	+10°	32.4	100°	+34°

The position found for the axes of preferential motion is in good agreement with other determinations which, according to the summary by Eddington, give $\alpha = 94°$, $\delta = +12°$.

The asymmetrical terms, or those of odd order, are next determined separately for the three groups. If those which depend only on the galactic latitude are considered, we obtain expressions for θ from which the following maximum values may be derived:

GROUP I		GROUP II		GROUP III	
ϑ	b	ϑ	b	ϑ	b
km. 13.9	−19°.1	*km.* 19.7	−19°.9	*km.* 28.4	−22°.8

We find, accordingly, in all cases a negative latitude for the maximum θ. It is known, however, that the sun is situated north of the real galactic plane, the distance being about 20 parsecs,[7] or a distance corresponding to a parallax of 0".05. This indicates a maximum of motion, not in the galactic equator, but in the real galactic plane. The form of the surface for the second group is shown in figure 1.

The surfaces which represent the average radial velocity for the first and second groups are very nearly the same, the most important features being two large terms of second and third order. These are as follows, b and l being the galactic latitude and longitude, respectively:

	SECOND ORDER	THIRD ORDER
Group I	$2.59 \cos^2 b \cos 2 (l - 174°.3)$	$2.90 \cos^3 b \cos 3 (l - 76°.0)$
Group II	$3.18 \cos^2 b \cos 2 (l - 162°.7)$	$3.10 \cos^3 b \cos 3 (l - 77°.6)$

The second order terms define the stream motion, and the existence of the large third order terms shows that the axes of maximum radial velocity do not lie in a straight line.

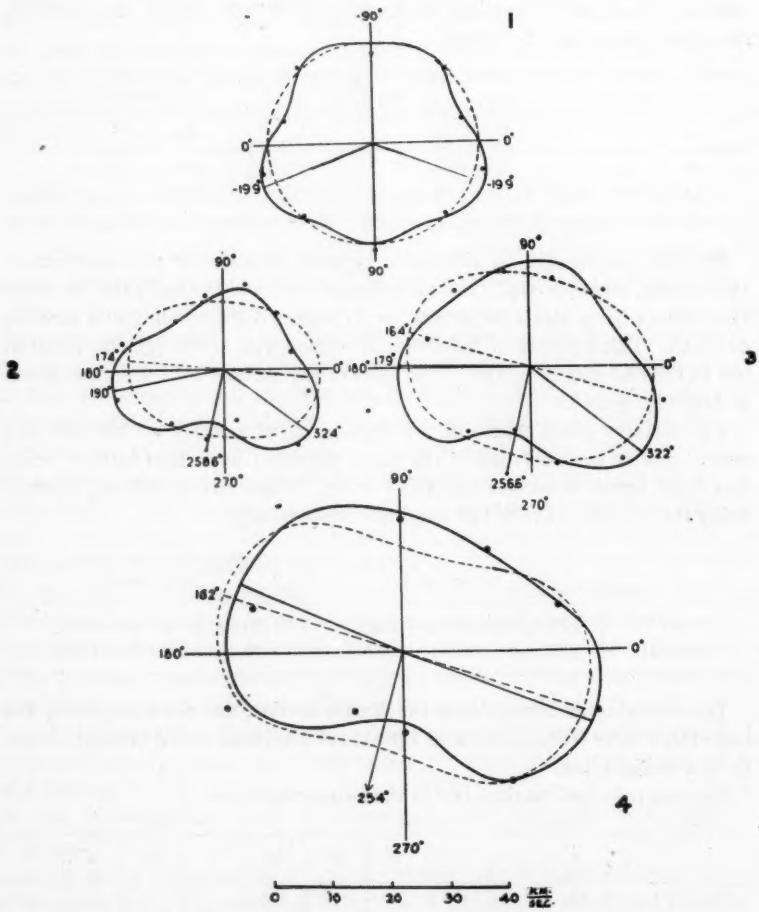
The two principal maxima of θ in the two surfaces are

	ϑ	l	b	α	δ	ϑ	l	b	α	δ
	km.					*km.*				
Group I	19.60	190°	+ 4°	109°	−7°	17.50	324°	− 2°	264°	−33°
Group II	24.23	179	−18	84	−7	26.33	322	−19	283	−41

We find, therefore, that the axes of largest radial mobility, which may be assumed to correspond approximately to the direction of preferential motion in space, form obtuse angles equal to 134° and 128°, respectively, for the two

groups, and that there is a pronounced minimum at longitude 258°. The nearest approach to a symmetrical plane at right angles to the galacticplane has the longitude

258°.6 for Group I and 256°.6 for Group II.



In figures 2 and 3 are shown the intersections between the radial velocity surfaces and the galactic equator. The dotted curves indicate the intersections when only terms of even order are considered, and the arrows mark the position of the symmetrical planes.

The properties of these surfaces may be explained on the assumption that

the motions of the stars depend upon their positions relative to the center of the galactic system. According to Charlier[3] the position of the center as derived from 800 B-type stars is

$$l = 236° \qquad b = -14° \qquad \text{Distance} = 88 \text{ parsecs.}$$

O. R. Walkey[8] finds from 30736 stars of all types the longitude $l = 246°$. These values of the galactic longitude of the center of the stellar system are in fair agreement with the value 258° found for the position of the symmetrical plane. We may, therefore, conclude that the variation of average radial velocity with direction is due probably to a motion of the stars around the center of the galactic system. Moreover, if the stars are moving around the center of the stellar system we should expect a minimum orbital velocity near the center and a maximum at a certain distance from the center.[5] The minimum in radial velocity near longitude 268° is in agreement with this conclusion.

In the case of the stars nearest to us, we should expect the axes of preferential motion to lie nearly in a straight line, since their distance from us is small as compared with their distance from the center.[9] To test this question I have made an analysis of the radial velocities of the stars in Group III, which contains the nearest stars, combining all stars between galactic latitudes −66° and +66°. The axes of maximum radial velocity for the resulting curve have the longitudes 157° and 340°, which thus differ by 183°. The longitude of the axis of symmetry is 254°, a value in good agreement with that found for the more distant stars. The intersection of the surface with the galactic plane is shown in figure 4.

The conclusion to be drawn from these results is that stream motion is probably a local effect caused by a preferential motion of the stars in both directions around the center of the stellar system. This might have been expected from the fact that the motions of the two 'drifts' have been found to be in the galactic plane and at right angles to the direction of the center of the galaxy. The deviation of the axes of preferential motion from a straight line furnishes strong evidence in support of this conclusion.

[1] Adams, W. S., and Strömberg, G., *Mt. Wilson Contr.* No. 131, *Astroph. J., Chicago*, **45**, 1917, (293–305).

[2] Kapteyn, J. C., *Groningen, Pub. Astr. Lab.*, No. 8.

[3] Turner, H. H., *Mon. Not. R. Astr., Soc. London*, **72**, 1912, (387–407).

[4] Eddington, A. S., *Ibid.*, **75**, 1915, (366–376).

[5] Mt. Wilson Solar Observatory, *Annual Report*, 1916, (255).

[6] Eddington, A. S., *Mon. Not. R. Astr. Soc., London*, F. S., 1915, (521–530).

[7] Charlier, C. V. L., *Medd. Lunds Obs., Upsala*, Ser. 2, No. 14, 1916, (31).

[8] Walkey, O. R., *Ibid.*, **74**, 1914, (649–655).

[9] Strömgren, E., *Astr. Nachr., Kiel*, **203**, 1916, (17–24).

DISEASE RESISTANCE IN CABBAGE

By L. R. Jones

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Communicated by R. A. Harper, December 31, 1917

The cabbage, while ordinarily one of the most vigorous of cultivated plants, is highly susceptible to certain parasitic diseases. The most destructive of these, known as 'yellows,' is due to a soil inhabiting fungus (*Fusarium conglutinans*) which invades the root system. Once introduced this parasite may persist indefinitely in the soil rendering it 'cabbage sick' so that this crop can no longer be grown upon it. One who knows the nature of this parasite and its manner of host invasion would have little hope of its practical control through any of the usual methods of spraying or seed or soil treatment. Extensive experimental trials have justified this conclusion.[1] On the other hand, when we were called upon in 1910 to investigate this disease and its control in Wisconsin, it was these very factors which encouraged the hope that it might be possible to secure disease resistant varieties or strains. The principle involved in such an undertaking is, of course, that of 'the survival of the fittest.' The things essential to any satisfactory progress are that there be among the host plants well marked variations in individual susceptibility or 'resistance' to the parasitic attacks and that this character, whatever its cause, be fixed and transmissible from generation to generation. Another very important matter, from the practical standpoint, is that such resistant characters be combined with those which give the plants economic value. Examination of the worst diseased fields led to the discovery of certain individual plants which developed apparently normally although all neighboring plants were dying or dead from the effects of the parasite. Moreover, among these survivors were to be found representatives of the best commercial types. The first and last conditions were therefore met. Would these characters persist in seed obtained from such plants?

Somewhat over 50 such resistant individuals were selected in the autumn of 1910, seed was grown from them in 1911, and in 1912 this seed planted, each 'head strain' separately, on the 'sickest' available soil. Omitting all details the general results may be summarized by the statement that in every case the selected head strains transmitted in considerable degree their resistant qualities, and certain of them did so in high degree. Thus of the control (non-selected) varieties the majority were killed by the parasite and most of the balance were so diseased that they failed to form heads, whereas the *poorest* of the selected head strains proved decidedly superior to the *best* of these controls, and of the best selected head strain 98% of the plants lived through the season and 93% of them formed heads. The figures which may best be cited are, however, those comparing the head strain which in the behavior

of its progeny has since proved to be the best with the unselected control strains.

Selected head strain	96% lived	80% headed
Non-selected controls	36% lived	16% headed

From the best of these heads, selected as the second generation survivors on sick soil in 1912, seed was grown in 1913 and like trials on the same infected soil made again in 1914. In order the better to show the significance of these results the following summary includes the averages of *all* of the head strains selected from the best strain in 1912 together with the results from the *best* head strain. Data are also here included as to the per cent of diseased plants in each strain at the end of the season, the average weight of the heads, and the computed yields in tons per acre.

SECOND GENERAL TRIALS, 1914	LIVED	HEADED	DISEASED	WEIGHT PER HEAD	YIELD PER ACRE
	per cent	*per cent*	*per cent*	*pounds*	*tons*
Average all selected strains	99.5	94.0	5.0	4.0	12.2
Best selected head strain	100.0	98.0	1.5	5.5	18.8
Average non-selected controls	46.0	24.5	81.0	2.7	2.1

A comparison of the 1914 results with each other and with those of 1912 shows clearly certain encouraging things. First, the disease resistant character, whatever may constitute it, is fixed and inheritable; second, there is a distinct improvement in the second generation as compared with the first; third, there is enough variation as between the selected head strains of the second generation to furnish a basis for further possible improvement by continued selection. Perhaps this last idea requires restatement in such a way as to lay the emphasis upon another fact. Since there is a considerable variation within the progeny of even the selected head strains it is necessary to continue the method of growing the seed strains on 'sick' soil for trial and selection in order to maintain the highest standards, and were this to be discontinued there would probably be a tendency toward gradual reversion. Such selections and trials have since been continued in an intensive way by our department with further encouraging results while the more extensive work of seed growing and its distribution (under the name Wisconsin Hollander) has passed to the hands of a committee of practical cabbage growers organized for this purpose.

In this way our own departmental activities, including the investigations of certain graduate students, have been freed for direction to some of the more fundamental questions involved. Among these the following may be defined.

1. Wherein is the difference between the susceptible and the resistant plant? In other words, what constitutes disease resistance?

FIG. 1. A FUSARIUM-SICK CABBAGE FIELD

Note that while most of the plants are diseased or dead certain appear normal. Selections of such were made from this field in 1910.

FIG. 2. A TRIAL OF RESISTANT STRAINS ON 'SICK' SOIL IN 1916

The central row, nearly all dead, is a commercial non-resistant strain planted as a control. The remainder of the field is planted with selected resistant strains, the first row at the left being recently selected kraut varieties, Brunswick and All Seasons, the rest being Wisconsin Hollander.

2. How does this factor of disease resistance behave in inheritance? Is it surely transmissible and does it 'Mendelize?'

3. Is the quality of resistance or susceptibility influenced by environmental factors, if so, how? Related to this is the question as to whether such resistance will persist when the strain selected in one locality, e.g., Wisconsin, is grown in a remote locality, e.g., New Jersey.

4. Is the principle general? In other words, is there similar variation in individual susceptibility and resistance to this parasite among other varieties of cabbage than the one considered above? How general is the occurrence of such differences with other host plants and other plant parasites?

While wholly satisfying answers are not yet formulated for any of these questions, such evidence as has been secured may be summarized as follows.

As bearing on the first question, W. H. Tisdale, working in our laboratory, has sought to learn what difference there is in the relations of the parasite to the resistant and the susceptible plants. Since the cabbage is a clumsy plant of slow development he used for his primary investigations the flax which is invaded by a closely similar parasitic Fusarium and which has well differentiated resistant and susceptible strains. Secondary comparative studies with the cabbage, while not yet complete, indicate a general likeness in behavior. He has learned that the difference in resistance is not due to any superficial obstacle since the parasite, which may enter through the root hairs, penetrates these freely in the resistant as well as the susceptible plants. The difference is in the relations of the interior cells of the host and the parasite. In the suceptible plants the parasite penetrates directly to the vessels and then ramifies through them to the destruction of the host. In the resistant individuals, on the other hand, the invasion advances more slowly and before it reaches the vessels is checked and permanently walled off by the development of a corky layer. This reaction may be interpreted in various ways but the one we favor is that the resistant tissues have the ability to restrain the development of the parasite to a greater degree than do the susceptible and so give time for the protective cork formation.

With reference to the second question, Tisdale[2] has also sought to follow the behavior of resistance as an inherited character again using the flax. Biffin, in England, working with wheat, concluded that resistance to the rust parasite (Puccinia) was inherited as a simple Mendelian character, being recessive. With flax the problem proves not so simple. Crossing highly susceptible with resistant strains and testing the progeny shows that resistance is clearly a transmissible character, but the hybrids are in general more or less intermediate in this respect with a tendency for resistance to be dominant. Much variation is found in lines originating from different parents. The indications are that resistance is not a single character but a complex, dependent upon a number of heritable factors.

As to the third question, environmental factors do have a marked influence in determining whether the Fusarium parasite will invade the cabbage. J. C.

Gilman working at Wisconsin and later at the Missouri Botanical Garden found that there is a 'critical soil temperature,' about 17°C., for such invasion. Below this the plants are not parasitized, even in the sickest soils while for some 10° above this the attack becomes progressively more virulent. Correlated with this is the experience that at lower soil temperatures, e.g., in cool summers, there is little disease even with susceptible strains and in hot summers there is a certain amount of infection even with our most resistant strains. It is theoretically possible, therefore, that a strain which was resistant in a relatively cool climate might prove susceptible when transferred to a warmer region. As a matter of field trial, however, these resistant Wisconsin cabbage strains have proved similarly resistant under trials extending from New Jersey to Iowa.

Regarding the fourth question we may speak with considerable confidence also. Our earlier work was confined, as previously suggested, to one cabbage variety, the Hollander. Selections which have since been made from other varieties, both in connection with our own investigations and by others in Ohio, Iowa and Maryland, have given encouraging results. We may be reasonably confident, therefore, that a Fusarium-resistant strain of cabbage may be secured from any vigorous established variety.

If we do not ask for too much in the way of practical application we may safely go farther in the generalization as to the occurrence of disease resistance in plants. Plants are always varying of course, and this includes variations in those factors which make them more or less susceptible or resistant to the attacks of any parasite. Unquestionably constantly increasing recognition of this principle will be given by plant breeders and plant pathologists in seeking to control plant diseases. On the other hand, experience indicates that with parasites of certain kinds including the Fusariums, it is relatively easier in practice to secure disease-resistant strains of host plants than it is with many other parasites, for example the common wheat rust.

[1] For a more detailed consideration of these matters reference may be made to *Wis. Agric. Exp. Sta. Res. Bul.*, 38, The Control of Cabbage Yellows through Disease Resistance, by L. R. Jones and J. C. Gilman, December, 1915.

[2] Tisdale did his work on inheritance as a graduate student under the direction of Dr. L. J. Cole in the department of experimental breeding, at the University of Wisconsin. His results along these two lines have been formulated for publication in the *Journal of Agricultural Research*.

IS A MOVING STAR RETARDED BY THE REACTION OF ITS OWN RADIATION?

By Leigh Page

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Communicated by H. A. Bumstead, January 21, 1918

A question of some interest to the astronomer is whether or not a body in motion, such as a star, is retarded by the reaction of its own radiation. For, on the electromagnetic theory of radiation as developed by Maxwell and his followers, a beam of radiant energy is supposed to have a quasi-momentum, such that if a body emits energy in a single direction it will lose momentum and in consequence suffer a reaction tending to push it in the opposite direction. Now if a star is at rest, and in thermal equilibrium, it follows from symmetry that it will radiate equally in all directions, and there will be no resultant impulse. If, however, the star is in motion, classical electrodynamics leads to a greater emission in the forward direction than in the backward, and consequently it would appear at first sight as though there should be a retardation which would ultimately bring the star to rest. The problem has been treated in some detail by Professor Sir Joseph Larmor in the *Proceedings of the Fifth International Congress of Mathematicians*,[1] held at Cambridge in 1912, and he finds the resistance to motion due to the radiation to be

$$F = -vR/c^2$$

where v is the velocity of the star, R the rate of emission of energy, and c the velocity of light.

Apart from its intrinsic interest, Larmor's result is of importance in that it would constitute, if correct, a contradiction between classical electrodynamics and the Principle of Relativity (reference here is to the relativity of constant velocity systems, not to the broader conception of general relativity recently developed by Einstein). It is known, however, that the connection between classical electrodynamics and the Principle of Relativity is very close. Lorentz obtained the relativity transformations in his effort to give the electrodynamic equations for a moving system the same form as for a fixed system, even before Einstein advanced the relativity idea, and the author has shown that the electrodynamic equations can be derived in their entirety and exactly from the kinematical transformations of relativity and the assumption that each and every element of charge is a center of uniformly diverging tubes of strain.[2]

Now to calculate rigorously from the electrodynamic equations the reaction on so complicated a mass as a star would be hopelessly involved. Fortunately, however, the problem can be simplified to the extent of dealing with

a single oscillator, i.e., a single vibrating electron, and yet we can obtain a result that will be a perfectly general test of Larmor's expression. For the latter gives the retarding force as a function of the rate of total radiation and the velocity of the radiating body, and of these quantities alone. Hence if the ether exerts a reaction on a group of moving oscillators, it will exert a similar reaction on a single oscillator; and conversely, if there is no reaction on a single vibrating electron due to its drift velocity, there can be none on a group of such vibrators.

A rigorous solution of the problem for this relatively simple case shows the existence of no retarding force. Larmor's result is found to be invalid because of a tacit assumption underlying his reasoning which was introduced substantially in the following manner. In order not to complicate matters by the introduction of terms in the inverse second power, the radiation reaction is calculated by applying the electrodynamic equations to the surface of a moving sphere with the electron as center, whose radius is large compared to that of the electron (though small compared to a millimeter). In this way only those terms in the expressions for the electric and magnetic fields which involve the inverse first power of the distance from the electron need be retained. But the result obtained really gives the force on the electron and the ether inside the moving sphere, not that on the electron alone. Now, if the motion of the electron were undamped, the field inside this moving sphere would remain unchanged, and consequently the force found by Larmor would be that actually exerted on the electron. But as the electron is radiating, its motion must be damped unless energy is supplied from some outside source, and in that case it must be shown that no impulse accompanies the transfer of energy to the electron—a matter of considerable difficulty to treat rigorously. It is far simpler to consider the case of an oscillator left to itself and allowed to radiate at the expense of the energy of its vibration. For this case it is found that the force exerted on the electronic vibrator by the ether inside the moving sphere mentioned above is exactly equal and opposite to that due to the ether outside. Moreover, from the point of exchange of momentum, the law of conservation of momentum demands that

Momentum lost by electron = Momentum gained by ether outside sphere − Momentum lost by ether inside sphere.

The terms on the right hand side (the second of which is overlooked by Larmor) annul one another. Therefore a single moving oscillator is not retarded by its radiation field, and as already noted we can generalize this result and conclude that a moving body of any size and complexity suffers no retardation as a result of its emission of radiant energy.

In the analytical reasoning leading to this conclusion it is found necessary to develop the complete dynamical equation of the Lorentz electron through terms of the fifth order, for the most general type of motion. Previous derivations of this equation have been confined to some special case, such as

quasi-stationary motion in a straight line. The general equation is here published for the first time:

$$F_x = \frac{e^2 f_x}{6\pi a c^2 (1-\beta^2)^{3/2}} - \frac{e^2 \mathbf{f}\cdot\boldsymbol{\beta} f_x}{2\pi c^4 (1-\beta^2)^3} \cdots - \frac{e^2 \dot{f}_x}{6\pi c^3 (1-\beta^2)^2}$$
$$+ \frac{e^2 \ddot{f}_x a}{9\pi c^4 (1-\beta^2)^{5/2}} - \frac{e^2 \dddot{f}_x a^2}{18\pi c^5 (1-\beta^2)^3} \cdots$$

$$F_y = \frac{e^2 f_y}{6\pi a c^2 (1-\beta^2)^{1/2}} - \frac{e^2 \mathbf{f}\cdot\boldsymbol{\beta} f_y}{2\pi c^4 (1-\beta^2)^2} \cdots - \frac{e^2 \dot{f}_y}{6\pi c^3 (1-\beta^2)}$$
$$+ \frac{e^2 \ddot{f}_y a}{9\pi c^4 (1-\beta^2)^{3/2}} - \frac{e^2 \dddot{f}_y a^2}{18\pi c^5 (1-\beta^2)^2} \cdots$$

where the X axis is taken in the direction of motion, the Y axis is in any direction perpendicular to the velocity, a stands for the radius of the electron, f is its acceleration, and $\beta = v/c$ where v is the velocity of the electron and c that of light. The charge e is expressed in Lorentz's unit. It may be remarked that the form of this equation, in so far as it involves β, is in exact accord with the Principle of Relativity.

The analytical reasoning involved in obtaining these results from the electrodynamic equations will be given in a paper which has been submitted for publication to the *Physical Review*.

[1] *Proc. Fifth Intl. Cong. Math.*, 1, 1912, (207).
[2] Relativity and the Ether, *Amer. J. Sci., New Haven*, 38, 1914, (169).

ON ELECTROMAGNETIC INDUCTION AND RELATIVE MOTION. II.

By S. J. Barnett

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Communicated by R. A. Millikan, January 19, 1918

1. If a cylindrical condenser is placed in a uniform magnetic field with lines of induction parallel to its axis, and is short-circuited and rotated, together with the short-circuiting wire, about this axis, it becomes charged to a potential difference equal to the motional electromotive force in the wire, or the rate at which the wire cuts magnetic flux. If, however, the condenser and wire are fixed and the agent producing the magnetic field rotates, the relative motion being the same as before, the condenser does not become charged, as was proved[1] by precise experiment in 1912. This is the first case in electromagnetic induction in which the observed effect does not depend entirely on the relativity of the motion.

Without, however, the introduction of hypotheses unproved by experiment it does not seem possible to determine from the experiments on rotation what would happen in the case of translatory motion, or to obtain from them any answer to the question of the existence of the aether. The present investigation was undertaken with the hope of shedding some light upon these problems.

2. If an air condenser with horizontal parallel plates, short-circuited and placed in a uniform magnetic field whose lines of induction are parallel to the plates, is moved, together with the short-circuiting wire, in a direction parallel to the plates and perpendicular to the lines of induction, it will become charged to a potential difference equal to the motional electromotive force, which will be denoted by E, in the wire, the charges (provided, as will be assumed, that edge effects are negligible) being restricted to the inner or opposing faces of the plates.

3. If the region below the plane of the upper surface of the lower condenser plate were filled with a medium of zero permeability, the tubes of magnetic induction would be confined to the region above this plane, and the condenser would become charged exactly as before for the same motion.

4. If, with the arrangement of §3, the condenser and wire were to remain fixed and the agent producing the magnetic field were to move, the relative motion being the same as before, the results to be expected would be different according to the hypothesis adopted with respect to the aether:

A. If there is no aether, and the principle of relativity is valid, the condenser would become charged exactly as in §2 and 3.

B. If the aether exists, the electric intensity produced by the motion of the tubes of induction in the aether between the condenser plates would be equal and opposite to the field intensity there due to the potential differences produced by the electromotive force in the short-circuiting wire, and *there would thus be no charge on the lower condenser plate.*

Thus we should have a method of discriminating between the two hypotheses.

5. There being of course no medium of zero permeability in existence, I have tried to secure an arrangement equivalent to that of §4, so far as the effect under investigation is concerned, by using an artifice analogous to the electric guard-ring.

Two similar electromagnets, referred to as the upper and lower magnets, with their coils in series are used to produce the field. The four poles are all alike, each being a rectangular parallelepiped with the largest sides horizontal and parallel to the condenser plates. The lower magnet and condenser are fixed to the floor. The upper magnet forms the bob of a huge pendulum swung from the ceiling, and has translatory motion parallel to the condenser plates when in its lowest position. When the upper magnet is in this position, the upper and lower poles are about a centimeter apart, and the upper surface of the lower condenser plate is symmetrically located with reference to all

the poles and the other parts of the magnets. The (now) uniform field containing the condenser is conceived to be divided into two parts as follows: an approximately fixed field beneath the plane of the upper surface of the lower plate, and above this plane a field whose tubes of induction move approximately with the speed of the upper magnet. The induced electromotive forces are thus restricted to the same regions as in the imaginary experiments of §4.

6. In the experimental arrangement the lower condenser plate is (ordinarily) connected by a wire with one quadrant pair of an electrometer. The other pair and the upper plate are connected (the latter through a key at will) to a thin metallic case forming a practically complete electric screen about the condenser-electrometer system. The wire joining the condenser and electrometer can be connected to the case by closing an electrically operated key K, thus short-circuiting both condenser and electrometer. A suitable calibrating arrangement is provided.

7. The calibration experiments give the deflection D which would be produced by charging the condenser to the potential difference E of §2 (the upper plate being disconnected from the case for the purpose), insulating the lower plate and electrometer by opening K, and reconnecting the upper plate to the case.

8. The principal experiment, together with experiments for determining extraneous effects, gives the deflection d due to any charge on the lower plate produced when the upper magnet moves (with known speed) past the center of the fixed system, opening the key K at the center of the motion. An extended series of experiments shows that d/D is zero within the limits of the experimental error.

The investigation thus appears to support the hypothesis of §4, B, which assumes the existence of the aether, and to be inconsistent with the principle of relativity.

I am indebted to Mrs. Barnett for a great deal of help in making the experiments, to Mr. Freund for mechanician's assistance, and to the Carnegie Institution and Professor Pegram for some of the instruments used.

A detailed account of the work will be submitted for publication to the *Physical Review*.

[1] *Barnett, S. J., Physic. Rev., Ithaca, N. Y.*, **35**, 1912, (323–336)

NATIONAL RESEARCH COUNCIL

REPORT OF THE COMMITTEE ON ANTHROPOLOGY

The Committee on Anthropology was one of the first constituted under the National Research Council. Its personnel is limited to representatives of Physical and Medical Anthropology which branches it was recognized could be of direct assistance in national preparedness. Its membership comprises William H. Holmes, Chairman; Ales Hrdlicka, Secretary; Dr. Charles B. Davenport, Carnegie Institution; Dr. Frederick L. Hoffman, Chief Statistician, The Prudential Insurance Company; Dr. E. A. Hooton, Anthropologist of Harvard University; Dr. George M. Kober, Dean of the Medical Department, Georgetown University; Mr. Madison Grant, Trustee, American Museum of Natural History; and Dr. Tom A. Williams, Psychiatrist and Anthropologist.

Activities of the Committee.—On February 16, 1917, certain recommendations were formulated, which under the heading "A National Anthropometric Survey," were submitted to the Executive Committee of the National Research Council by Dr. Charles D. Walcott, first vice president of the Council.

In April additional suggestions were laid before Dr. V. C. Vaughan, representing the Council. These suggestions related to changes in the physical requirements of the Army, the advisability of proper regulation of the instruments and methods to be used in the measurement of recruits, the need of additional anthropological observations on drafted men in the camps, and the preservation of valuable data and specimens on post-mortem material. Later these recommendations were formally submitted to the National Research Council.

In May these suggestions were submitted in final form. In June they were presented before the Medical Branch of the National Defense Council, and in August were published in brief form in the *Proceedings of the National Academy*. In June the Committee made further definite proposals for the standardization of the instruments used in the examination of recruits and for the regulation of methods.

Finally during August and September a proposal was made for sending a trained anatomist to the base hospitals in Europe for the purpose of collecting much needed observations as well as specimens for further study. At the same time the Committee called attention to the probability that our men in France would discover in their digging operations valuable archeological and skeletal material, which should be preserved to science; and further, the Committee offered its services in dealing with the many problems relating to race, nationality and language which are bound to be encountered

by the Commission that will have charge for the United States of peace negotiations.

The results of the activities of the Committee unfortunately are not wholly encouraging. It finds on the one hand a number of simple, practical and from every standpoint desirable steps to be taken, and on the other the great difficulty of securing favorable action by the proper authorities. The only recommendation of the Committee so far adopted is that relating to the necessity of reducing the minimum physical requirements in recruiting. Practically nothing has been done towards the improvement of the methods and instruments employed in the examination of drafted men. The blanks used for the examination of recruits are essentially the same as of old. The *Manual of Instructions* for examiners was printed but no directions were included respecting the methods or instruments to be used in the measuring. The Committee has found that a large percentage of the instruments used for the examinations are more or less defective; that the methods of taking the measurements are regulated by individual opinion, necessarily resulting in many errors; and that in cases the measuring is relegated by the examining physician to enlisted men, without adequate supervision. It is painfully evident that no improvement has been effected in this important matter since the Civil War, and that the millions of measurements to be taken will be entirely unreliable for scientific purposes. Thus we lose demographic data of the greatest value; data which would have given to science and to government for the first time reliable information regarding the physical characteristics of the American people in different parts of the country, in different occupations, and under different environments.

During the latter part of August and the first part of September the Secretary of the Committee took up with Major Vaughan and Major Millikan, the Acting Head of the National Research Council, the question of sending a trained anatomist or anthropologist to Europe, to be attached to the American base hospitals and to take charge of the collecting of data and objective material relating to post-mortem cases. As many races will be represented in the United States Army, this step is of particular importance. Both normal and pathological specimens should in suitable instances be preserved, to be deposited for future studies in the National and the Army Medical Museums and possibly other institutions. It was recommended that the specialist sent should be a man of high training and experience and that he be commissioned in the Medical Service of the Army so that his work could be carried on in a regular manner and not dependent on favors. It was suggested by the Committee that if necessary the salary and working expenses of this officer might be provided for, but this suggestion met with a stumbling block in the disinclination of those who are concerned, to commission the specialist who might be recommended.

The next step suggested by the Committee relates to the preservation of

any finds of ancient human material, archeological or skeletal, that may be discovered by the American Army in their trench digging and excavations in France. France, as is well known, was the home par excellence of man from the haziest antiquity. Most of our knowledge relating to his evolution has come from collections made in France, and much more material doubtless still lies in French soil. During the extensive digging of trenches and other excavations the men of our Army are more than likely to come across such precious remains and steps should be taken for their preservation. The armies on the western front have already made such discoveries.

Notwithstanding the discouragement so far encountered by the Committee a great deal in the line of its recommendations can still be accomplished. Many more drafted men will be examined and the opportunities for observations in the camps are multiplying. The collection of post-mortem data and material can be initiated at any time before the actual fighting commences. Our great problem now is how shall we proceed in order to be more successful in these directions in the future than we have been in the past, and how can our recommendations be made effective?

W. H. HOLMES, *Chairman.*
ALES HRDLICKA, *Secretary.*

NOTICE OF BIOGRAPHICAL MEMOIRS

The following biographical memoir has been published by the Academy since the last notices of such memoirs appeared in the August 1917 number of the Proceedings.

JOHN SHAW BILLINGS (1838–1913). By S. WEIR MITCHELL, With The Scientific Work of John Shaw Billings. By FIELDING H. GARRISON. *Biographical Memoirs of the National Academy*, **8**, pp. 375–416.

The first biographer sketches in generality, and with the sympathy of a personal friend, the life work of John Shaw Billings in its early struggles, in the army during the Civil War, in the Army Medical Museum, in the National Academy, and in the Astor-Tilden-Lenox Library of New York. Dr. Garrison, though not neglecting personal characteristics, treats primarily the achievements of Dr. Billings in the six fields of his activity, military and public hygiene, hospital construction and sanitary engineering, vital and medical statistics, medical bibliography and history, advancement of medical education and the condition of medicine in the United States, and in civil administration; he also gives an account of the scientific contributions originating with Dr. Billings either directly or through his pupils, but attempts no formal bibliographical list of the published writings.